upon their final termination, and this in these cases requires elucidation. Although we have treated a large number, and had many under our care for many months, we feel ourselves as yet quite in the dark as to the final results in gunshot wounds of the thorax.

In concluding this review, it may not seem out of place to give expression to our feeling of deep regret that the able and highly intelligent surgeon, who had collected with indefatigable industry so many valuable materials for the surgical history of our war, and who had made great progress towards the completion of the work, has not had the privilege afforded him of finishing his task. Had this been done, the profession could look with greater confidence for a full account of what medicine has done in its bloody fields and crowded hospitals, sure that, so far as we are concerned, it should not be said of this dreadful war, *caruit vate sacro.* W. F. A.

ART. XVIII.—*Lectures on Epilepsy, Pain, Paralysis, and certain other Disorders of the Nervous System, delivered at the Royal College of Physicians in London.* By CHARLES BLAND RADCLIFFE, M. D., Fellow of the Royal College of Physicians in London, Physician to the Westminster Hospital, &c. London: John Churchill & Sons, 1864.

A PHYSICAL theory of innervation and muscular action must, if established, be a grand advance in physiology. If its application to pathology and therapeutics be direct, and, most of all, if it tend to cause a revolution in practice, such a theory must have great importance. Has Dr. Radcliffe furnished us, in the work entitled as above, with this desideratum? Not without some sympathetic enthusiasm for the inquiry, excited by his own manifest though intelligent zeal, we have endeavoured to do justice to this question, by a careful study of the work. If our conclusion be not such as to satisfy either its author or our readers, it will not be from want of attentive consideration of the argument.

Twelve years sooner, as Dr. Radcliffe states in his preface, he published an essay on the "Philosophy of Vital Motion;" and afterwards other volumes, in which he endeavoured to show that it was necessary to revise the current theory of muscular motion, and to consider muscular contraction as "a physical process in which the attractive force inherent in the physical constitution of the muscular molecules was the contractile agent;" and, in consequence, that a radical change was called for in the pathology and therapeutics of convulsion, tremor, and spasm. Since the last[1] of these works was printed, he has found occasion to add further evidence in favour of his doctrine, and to extend its application to the physiology of sensation, and the therapeutics of pain and paralysis.

In the essential idea upon which he starts in all of these investigations, others had anticipated him to some extent. Dr. West, of Alford, as mentioned in Dr. Radcliffe's appendix, maintained distinctly, in 1832, that "the nervous influence present in relaxed muscular fibre is the only influence which the nerves of volition possess over that tissue; that its office is to restrain or control the tendency to contract which is inherent in the muscle:

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[1] Epileptic and other Convulsive Affections of the Nervous System, and their Treatment. Incorporating the Gulstonian Lectures for 1860.

and that contraction can only take place when by an act of the will this influence is suspended, the muscle being then left to act according to its own innate properties." A little later, Sir Charles Bell is said to have *hinted* at a similar idea in one of his lectures, although premising an expectation that "the question could never be settled." Dugès of Montpellier, Matteucci, Engel of Vienna, and Stannius of Rostock, are also quoted as advocating more fully a hypothesis of the same kind.

Our author's clear language may be best used to indicate what the facts and arguments of his lectures are designed to antagonize.

> "With respect to muscular action, the current doctrine is, that muscle is endowed with a vital property of contractility; that muscular contraction is the sign of vital excitement in this property; that excessive muscular contraction, whether voluntary or involuntary, betokens excessive vital excitement in this property, and that the treatment required in order to subdue convulsion, or any disorder analogous to convulsion, is one which is calculated to quiet excessive vital excitement. What I hope to do is, to show that the facts, old and new, but particularly those which have been brought to light during the last fifteen or twenty years, necessitate a very different doctrine, and a not less different practice. With respect to sensation, the current doctrine is, that certain kinds of nerve-tissue are endowed with a vital property of sensibility, that sensation is the sign of vital excitement in this property, that pain, or any sensation analogous to pain denotes excessive vital excitement, and that the treatment of pain and sensations analogous to pain must be ruled according to this view, the proper means being those which are calculated to subdue vital excitement. What I hope to do is to show that the change in doctrine and practice which is demanded in the case of disordered muscular action is also demanded in the case of disordered sensation." (pp. 2, 3.)

First, then, the physiology of muscular motion is considered. Regarding animal electricity as essentially connected with this, our author gives a preliminary sketch of the successive discoveries of Galvani, Volta, Humboldt, Aldini, Nobili, Matteucci, and Dubois-Reymond upon that subject; whose importance is indicated by words cited from Humboldt, "La physiologie doit à Galvani et à Harvey ses deux bases principales." The joint labours of Matteucci and Dubois-Reymond have left no room for entertaining any doubt as to the reality of animal electricity. Matteucci has demonstrated its power to decompose iodide of potassium, and to affect the condenser and galvanometer, and has shown that muscular contraction is accompanied by an electrical discharge, analogous to that of the torpedo. Dubois-Reymond's discoveries go far into detail in regard to the electrical currents in nerve and muscle in different kinds of animals.

Dr. Radcliffe's first proposition is one of those established by the observations of the last named investigator; that "during inaction the natural state of living muscle and nerve is one in which the longitudinal and transverse surfaces of the fibres are in a state of electrical antagonism, the longitudinal surfaces being electrified positively, and the transverse surfaces negatively." In an electrical point of view, there is stated to be no difference whatever between nerve and muscular fibre, or between different kinds of muscular fibre, or of nerve fibre.

To explain this electrical antagonism between the surfaces, Dubois-Reymond proposes one hypothesis, and our author another, as to the molecular arrangement and polarity of muscle and nerve; the same for both. These different views cannot well be made intelligible without the diagrams given in Dr. Radcliffe's book. Why the molecules, or fibres composed of them, should preserve their particular electrical arrangement, is a problem yet unsolved.

Further, under certain circumstances the electrical relations between the longitudinal and transverse surfaces of muscle or nerve may be *reversed;* the longitudinal becoming negative, the transverse positive. This may happen just before rigor mortis in some animals, in the muscles; and in nerves after various injuries. In an isolated limb of a frog, contact of a hot iron with the nerve will cause it. Dr. Radcliffe asserts that—

> "The vital properties of the nerve are not materially affected so long as the reversal lasts; the muscles of the leg may very readily be thrown into a state of contraction by acting upon the nerve, while the needle of the galvanometer shows very plainly that the natural electrical relations of the longitudinal and transverse surfaces are reversed." (p. 26.)

Very distinct statement and illustration follow of other propositions. We have space for some of them only. During the state of inaction living animal tissues furnish signs of natural electricity of high tension. One of the most remarkable experiments upon this point is that of Humboldt; in which the spontaneous animal electricity (as he regarded it) was made to traverse a gap between the ends of a divided nerve to the extent of four-fifths of a line. Muscles *lose* their "natural electricity" in *rigor mortis,* at the same time that they lose their irritability.

Contrary to views elsewhere previously expressed by him, Dr. Radcliffe now believes that the primary electrical condition of living muscle and nerve during inaction, is that of statical electricity; the "muscular current" and "nerve current" being only secondary phenomena. Dubois-Reymond, however, thinks that strong currents circulate in closed circuits around each one of his peripolar molecules.

Now it appears to us, however abrupt the statement of this opinion may appear just here, that these decided differences, upon such a point, show how *purely hypothetical* very much of the foundation of this "theory of nerve and muscular action" is. The language of Dr. Radcliffe's next two propositions confirms this view. "It is possible," he says, that the elongated state of the fibres of living muscle may be due to the presence of the electricity which is inherent in them during the state of inaction. "It is possible" that the state of the muscular fibre in rigor mortis may be the necessary consequence of the extinction of the natural electricity of the fibre. Were these possibilities, and others consequent upon them, made certain, a physiological law would be founded, instead of a plausible conjecture.

Action in motor nerve and muscle are, in our author's second lecture, similarly studied. "The state of action in a muscle is accompanied by a discharge of electricity analogous to that of a torpedo." This is the starting proposition. Matteucci devised the experiment which proves it. A frog's leg, skinned, with all parts of the thigh cut away except the principal nerve, has this nerve laid upon the muscles of another leg prepared in exactly the same manner. When the nerve of the *latter* is pinched, "induced contractions" take place in the muscles of the former. Of course the force causing these secondary contractions has been communicated from the muscles of the limb acted upon. That it is *electrical* force is shown, further, by interposing small pieces of moistened lamp-cotton between the nerve of the one limb and the muscles of the other; when the same result will occur as before. That it is "analogous to the discharge of the torpedo" is inferred "from sundry obvious analogies between the anatomy and physiology of the muscular system and the anatomy and physiology of the electric system." (p. 42.)

Those analogies are close, undoubtedly. But, as Dr. Radcliffe afterwards founds a good deal upon this proposition, let us scrutinize it a little. What is proved should be carefully separated, in such an inquiry, from what is merely possible; a mere analogy, from identity. Observe, then, how *artificial* all the circumstances are under which the experiment is performed. Brevity requires us simply to ask, is *every* muscular action, in a living animal, accompanied by a "discharge analogous to that of the torpedo?" Can it be so, when we know that, although nerves and muscles lie packed together in all the limbs and other parts of the body, a few fibres, or the largest muscle, may act *alone*, with no "induced contractions" at all? May not the result described be merely pathological (traumatic); like so many of the complicated results of vivisection?

Further; in another part of the volume, this experiment and conclusion are made important parts of an argument to show that the relation between muscle (as well as nerve) in action, and electricity, is, a *loss* of electricity as the *cause* of the action; the natural electricity of the muscle maintaining its *dilatation*. Yet, in the experiment above, and in the corresponding one next mentioned in the book, where the nerve of one limb touches the nerve instead of the muscle of the other, the electricity discharged by the one limb is *received* by the other; and produces in it *not dilatation*, but *contraction*; the only *proof* of the "discharge" being the contraction so induced. Is not this enough to show that no very essential inference should, as yet, be drawn from this experiment?

Next, we have (p. 45) the proposition, most important, perhaps, of all, that "the natural electricity present in living muscle during the state of inaction is almost or altogether absent in the state of action." The same statement, precisely, is made in regard to the inaction and action of motor nerve; founded, in both cases, on galvanometric experiments.

> "M. Dubois-Reymond does not speak of this disappearance of electricity from the muscle, during the state of action, as a *discharge*. Indeed, he has his thoughts fully occupied with the idea of current electricity, and he ignores altogether the evidence of discharge which has been supplied by M. Matteucci. As it seems to me, however, M. Dubois-Reymond supplies the very proof which M. Matteucci requires to supplement his discovery; for in the case in which M. Matteucci infers the existence of electrial *discharge*, M. Dubois-Reymond shows that there is an actual *disappearance* of electricity." (p. 47.)

We have looked through the volume to find some allusion, in connection with this "disappearance of electricity" (which is said also (p. 124) to attend voluntary muscular action), to the theory of the transmutation of electrical into mechanical contractile force, so familiar to many physiologists since the publication of Grove's inquiries. It is found on page 132 of Dr. Radcliffe's book; and is by him very briefly disposed of as a "notion," which seems to him not in harmony with the history of *rigor mortis*; as, to his view, "the explanation, which will apply to ordinary muscular contraction, as well as to rigor mortis, is surely to be preferred to that explanation, which, at best, can only apply to ordinary muscular contraction." To this, we must say, "*non sequitur*." It involves an assumption, open to very great doubt, that rigor mortis and common contraction of living muscle are phenomena of one category, with one explanation. Nor is it at all impossible to account, hypothetically, for rigor mortis, in consistency at least with the theory of "conversion of force." The neglect of this theory, indeed, appears to us the weakest point in Dr. Radcliffe's book.

It would be improper to detain the reader with the minutiæ of the argument, as very clearly given by our author. His style is certainly a model of lucidity and precision. It would be injustice to attempt to condense his matter into less compass than that of his own pages, since that could only be accomplished by omission of important points. Mere reference to the most prominent difficulties presented must suffice us.

After establishing that "a motor nerve which has lost its natural electricity has also lost its irritability," some very curious effects are shown, as connected with the inclusion of a nerve between the poles of a galvanic circuit, according to the relative position of the positive and negative poles. Using for a time the common terms of "direct and inverse," or "centrifugal and centripetal currents," he afterwards speaks, in a manner less easily reconciled with prevalent views of electrodynamics, of "the action of *each pole, singly,* upon the nerve." We must confess to an inability to find either necessity or advantage in this employment of terms, over that of those which ascribe stimulation and paralysis, respectively, of a nerve, under the circumstances mentioned, to *current* action, either with or adverse to the polarity of the nerve-trunk itself. Either set of expressions is obviously consistent with the conclusion given (p. 75), that all these results "appear to be the natural consequences of the reaction which must take place between the artificial electricity on the one hand and the natural electricity on the other."

The influence of the blood upon the activity of muscles, nerve-centres, and nerves is the first topic entered upon in Dr. Radcliffe's third lecture. It is said, and proved by experiments by Brown-Séquard and Stannius, not only that rigor mortis is associated with stagnation and coagulation of the blood, but that muscles which have passed into the state of rigor mortis will return into the state of vital relaxation if their muscles be supplied with a sufficient amount of blood. The experiments, fully described, are decisive.

Next he asserts that "increased disposition to ordinary muscular contraction appears to be associated with diminished supply of blood to the muscular system." (p. 84.) All the facts mentioned on this point are such as show greater "irritability" of the muscles in animals (reptiles and fishes) whose blood is least arterialized, and in hybernating mammals than in the same at other times, and in a warm-blooded animal whose circulation was depressed to the reptilian standard by division of the cervical portion of the spinal cord. Two propositions follow which are generally admitted, that "general convulsion is brought about by sudden hemorrhage," and also "by suddenly arresting the arterialization of the blood," as by suffocation.

Harley's experiments are adduced to show further that air left in contact with blood to which strychnia or brucia has been added contains more oxygen and less carbonic acid than air left in contact with simple blood. Our author infers that "one way in which strychnia or brucia brings about spasmodic muscular contraction is by producing a change in the blood which is equivalent to loss of arterial blood." (p. 87.) He concludes also, very reasonably, we consider, that the influence of venous blood in the process of muscular motion is "equivalent to the absence of arterial blood" solely, instead of the carbonic acid contained in it being, as Dr. Brown-Séquard has urged, a direct stimulus to muscular action.

In some pages which follow, an almost sophistical confounding occurs, it seems to us, of *convulsive* action with "ordinary muscular contraction." No experiments illustrating to any considerable extent the process of normal voluntary motion are described; but whatever is shown to be true of rigor mortis, or convulsive or spasmodic (all, of course, morbid or pathological) muscular action, is taken for granted to be as true of all "ordinary muscular action." This should *not* be granted without proof; it does not have *a priori* probability. As to convulsion, spasm, and tremor, there need be no hesitation in admitting that "in the most exaggerated form" they may and do occur when "the amount of nervous influence supplied by the nervous centres to the muscles must be at or near zero." All the pathological conclusions of our author may be accepted and regarded of importance, without involving the sweeping physiological generalizations his terms require. Can voluntary muscular activity go on when nervous influence is at zero? or when the muscles are deprived of arterial blood? Our author's words sometimes negative these questions; as when he says that "a muscle left to itself is in a state of dilatation," and that "the functional activity of any organ is in proportion to its supply of arterial blood." We have, too, an experiment narrated (p. 98) in which there is "paralysis in the parts from which the blood is excluded, paralysis almost everywhere, convulsion nowhere."

It is needful for us to pass over many interesting pages, whose topics are so intricate that one must give them careful study to apprehend their bearing. The *controlling* influence of the brain, and probably medulla oblongata, in preventing morbid excito-motor disturbance under peripheral nervous irritation, is somewhat under-stated, although mentioned.

The "physical theory of muscular action" adopted by Dr. Radcliffe is expressed summarily as follows (p. 110):—

> "When a muscle is made to contract by means of its nerve, there is reason to believe that the electrical discharge (analogous to the torpedo) which accompanies nervous action has reversed the electrical relations of the exterior and interior of the muscular fibres in the part acted upon; that this reversal has led to the discharge of the electricity which is present in the muscular fibres during the time of rest, and which keeps these fibres in a state of relaxation so long as it is present; and that this discharge of electricity brings about muscular contraction by leaving the muscle free to yield to the action of the attractive force which is inherent in the physical constitution of the muscular molecules."

On this we must remark, further, that our author has proved (p. 26) that while the needle of the galvanometer shows that the natural electrical relations of the longitudinal and transverse surfaces of a nerve are reversed, the muscles of the limb experimented upon may be "thrown into a state of contraction by acting upon the nerve." That is, the reversal being for the time persistent, contraction may be otherwise produced and intermitted; of course, therefore, the one cannot depend upon the other.

Moreover, no explanation of the *source* of the electricity natural to muscle as well as nerve is given with this theory; the cause of the polarity or statical repulsion of the molecules in dilated muscle is not suggested. We have, then, for those molecules, two sorts of attributes assumed: 1st, an *electrical repulsion*, owing to the arrangement of the particles, they being alternately positive and negative; 2d, a *tendency to attract each other*, owing to their *physical constitution*. Is not this less simple, after all, and less satisfactory, than the supposition that dilatation of muscular fibre is owing to the elasticity which it has in common with several other tissues, and that contraction is due to attractive force developed in the molecules in the direction of the length of the fibre, whether it be by the conversion of electrical into mechanically contractile force or not? We are not wedded to this theory, but simply fail to find demonstrative reason for preferring the one above proposed instead of it. Dr. Radcliffe remarks himself that one objection to it is that *nerves* ought to contract in their length, as well as muscles, under the same reversal of the electrical relations of their surfaces which is observed to occur. Unable to obtain any instance of nerves so contracting, he is obliged to be contented with supposing it *possible* during life, and with the conjecture that the beaded appearance of nerves after death is of that nature. This does not seem to us to solve the objection.

Nor can we find that an actual advance is made by our author's reasoning upon the subject of rhythmical muscular motion, whose alternations he makes to depend on electrical reversals in ganglia, whose condition is again affected by the injection of arterial blood, and its becoming venous, successively. As a hypothesis it may do, but no better than other hypotheses.

It is certainly claiming too much to assert that such views explain "capillary force." While Dr. Radcliffe is no doubt right in considering that a force resides in the vessels (arteries) contributing to the circulation, this yet leaves over a force of a different nature, acting in the capillary region, analogous to that moving the sap in plants.

Much more might be said upon the very similar views set forth on the physiology of sensation. But, as the facts and arguments are parallel, nearly the same remarks would apply.

It is satisfactory to be able to speak with more unqualified approbation of most of the pathological discussions and conclusions of our author.

Of epileptic, epileptiform, and hysterical conclusions it is, on the basis of facts, asserted that they are "connected with depressed and not with exalted vitality." The signs of this condition are derived from the respiration and circulation, both during and between the paroxysms.

One incidental proposition admits of question (p. 184) :—

> "At the height of the epileptic or epileptiform paroxysm the pulse is usually full, strong, and frequent, because the arteries are then labouring under a load of *black* blood, as they are found to labour in suffocation, and not because these vessels are then receiving an increased supply of *red* blood."

We must doubt the dependence of the character of the pulse in this case, or in suffocation, upon *either* the venous or the arterial condition of the blood. Experiments of Dr. J. Reid, of Aberdeen, and Prof. Draper, the younger, of New York, are referred to as to the state of things in suffocation. The pulse was found, by the hæmadynamometer, to increase greatly after the windpipe had been tied. The *apnœal* pulse, therefore, Dr. Radcliffe infers, is the *pulse of black blood.* But Dr. Draper found blood accumulating, during suffocation, in the aorta. Might not obstruction, *resistance*, increase the hæmadynamometric pressure to the degree observed ?

It is correctly stated that convulsion never coincides with a state of active febrile excitement. Dr. Radcliffe also avers that there is no clinical evidence to show that, in convulsion, there is an over-active circulation in the brain. Nor are *post-mortem* appearances thought to conflict with this view. Schröder Van Der Kolk's observations of the large dilatation, with thickening of the walls of the bloodvessels of the medulla oblongata in epileptics is the most open to different interpretation upon this point. Congestion of the cerebral veins has, likewise, been shown not to be associated in causation with convulsions.

Therapeutical deductions of great interest are drawn from these facts; and it is on account of these especially that attention was called to Dr. Radcliffe's previous volume upon the same subject. He argues, with considerable force, backing his reasoning with clinical facts, against purging and low diet in epilepsy, and in favour of cod-liver oil, phosphorus, and fatty food; in commendation, too, of regulated gymnastic exercises; against belladonna and zinc, and for bromide of potassium, and alcoholic regimen. Sir Charles Locock is lauded for the introduction of the bromide of potassium in the treatment of epilepsy, as opening at once "a brighter future for the fortunes of epileptics." While experiments upon animals, and some clinical observations, give reason to ascribe to it narcotic and anæsthetic properties, the exact rationale of its action in epilepsy is not determined.

Phosphorus Dr. Radcliffe has used in the form of a mixture of the ethereal tincture of phosphorus of the French Codex, mixed (fʒj in f℥ij) with sulphuric ether; his dose of this preparation being half a fluidrachm or a drachm mixed with water.

Against belladonna he urges that its causing dilatation of the pupil is a contra-indication in epilepsy. This dilatation, he considers, "there is reason to believe," results from the belladonna producing an *anæmic* condition of the brain; the size of the iris depending, when in a state of rest, in great measure upon the fulness or emptiness of its bloodvessels. In epilepsy, therefore, where our author's theory requires the supposition of an anæmic tendency in the cerebral circulation, this action would be undesirable. Without having anything at all to urge in favour of the use of belladonna in epilepsy, and, at the same time, without being able to disprove this explanation, we must consider it very doubtful.

Less than a page, in this work, is given to the topic of the use of alcohol in convulsive affections. This is positively favourable, however. These are his words: "There is reason to believe that alcoholic stimulants are very trustworthy antispasmodics in the prevention and treatment of convulsion." (p. 249.) In accepting the truth of this as a therapeutical propositon, great judgment must be called for in its application. Few modes of practice could do more harm if indiscreetly carried out. It is impossible to doubt, nevertheless, that facts abound to show that it contains a valuable and often practically applicable truth.

The general statement which is the most important result of all of Dr. Radcliffe's inquiries, and which may well stand, whatever may be thought of his purely physiological views, is thus expressed: "There is reason to believe that the therapeutics of convulsion must be based upon the notion that vital power has to be reinforced, and not upon the contrary notion."

The last two lectures are occupied with the subjects of tremor, spasm, pain, and paralysis. Similar pathological facts, in regard to tremor and spasm, to those already reviewed as to convulsions, justify the same conclusions upon their therapeutics. The indication is in both to exalt, not depress "vital tone." The absurdity of imagining that promiscuous stimulation can accomplish this is, of course, put aside by the familiar history of delirium tremens.

Pain is described by Dr. Radcliffe as being "either of a neuralgic character, or depending upon tenderness;" the latter being only accidental, the former essential. Inflammation, he considers to produce pain "accidentally," by pressure of the swelling upon tender parts. Such a line of distinction, as our author observes, must be broken in some parts. Is it not too often broken to be descriptive at all? Which category will receive the pain of cancer; of chronic rheumatism; of the pains designated by Inman as *myalgia?* Here, again, we are most satisfied with Dr. Radcliffe's clinical and pathological generalizations. Neuralgic pain, is, as he states, apt to be associated with a deeply depressed condition of the circulation; and to be antagonized by hyperæmic excitement. Sydenham and Garrod are quoted as giving data for the extension of this account to the severe pain of gout; and facts are given of the same purport in regard to other forms of pain apparently connected with an inflamed condition of the part.

Besides confirming theoretically some common therapeutical experience, our author infers further that "the diet best suited for the neuralgic habit of body appears to be one which does not contain too much lean meat and too little fatty and oily matter." (p. 311.) This follows from the physiological doctrine that oleaginous material abounds in all nerve-cells as an essential constituent. Dr. Radcliffe thinks that he has found practical exemplification of its truth. He also believes sugar in excess to be harmful to the neuralgic, by producing an excess of lactic acid in the blood. The properly regulated use of alcoholic drinks is recommended as an essential part of the preventive and curative treatment of neuralgic pain. (pp. 313–14.)

> "I have no hesitation in saying that the proper use of alcoholic stimulants is at once the natural corrective of the neuralgic habit and the most trustworthy of all anodynes. I have repeatedly known a paroxysm of neuralgia prevented and cut short by a glass of hot grog, and the condition of the patient in other respects improved rather than damaged by the proceeding; and this is more than I could say of any other method of treatment; and I have too often seen the beneficial influence of rum and milk in the morning in correcting the neuralgic habit to have any room left for doubt upon this score."

Again, we say, this is a well-sustained principle of practice, but especially capable of abuse. There are some patients, at least to whom "rum and milk in the morning" would be more dangerous, though less distressing, than neuralgia. As to the decided preference expressed for coffee over tea as a daily beverage for the neuralgic, we cannot but indulge a good deal of doubt as to its justification.

Dr. Radcliffe's views in regard to the electrical condition of the system lead him to believe that it may be necessary to return to the original mode of using electricity as a therapeutical agent: for instance, by insulating the patient and charging him with positive electricity. As yet, his experience does not suffice to afford confident support to this opinion. A suggestion is further made that the induction-coil might be very beneficial in neuralgia, "provided only the operation be carried on long enough to bring about vascular reaction, long enough to bring on an artificial hot stage by paralyzing to a certain extent the vaso-motor nerves." (p. 321.) This must be looked upon as heroic practice; not without danger can it be to induce a "paralytic" condition other than the most transient anæsthesia, anywhere in the body. Dr. Radcliffe mentions having seen three cases which seem to give countenance to his notion.

In the pages devoted to paralysis, the most interesting point perhaps is the discussion of the significance of "early and late rigidity." In place of Dr. Todd's opinion that late rigidity is due to the shrinking of cicatrization producing a slow and lingering "irritation," Dr. Radcliffe considers it to be a sounder explanation that "late rigidity is the anticipation in the part of rigor mortis—*rigor mortis in vitâ*, and nothing less nor more." This view is plausible.

The use of electricity in paralysis is spoken of in terms similar to those already cited in reference to neuralgia. Here too the employment of "shocks of the coil-machine until the operation is followed by a sufficient degree of vascular reaction" is recommended, with the intention of affecting the vaso-motor nerves of the centres involved. There is evidently reason to fear that such a course would be over-violent, rather promoting disorganization than nutrition.

Altogether, both to the physiologist and the physician, Dr. Radcliffe's book is one of great interest. In its practical facts especially it deserves careful attention; and in the whole discussion of the subject—often intricate—he has shown powers of analysis and elucidation, such as are rarely met with amongst medical writers. When unable to agree fully with him, every reader must concede that he has at least made it possible always to understand his statements and arguments; and thus, yet more advanced conclusions, in an important inquiry, are made more probable in the future.

H. H.

ART. XIX.—*Acupressure; a new Method of Arresting Surgical Hemorrhage, and of Accelerating the Healing of Wounds.* By JAMES Y. SIMPSON, M. D., F. R. S. E., Professor of Medicine in the University of Edinburgh, and Physician Accoucheur to the Queen for Scotland, &c. &c. &c. With illustrations. Demi-octavo, pp. 580. Edinburgh, 1864.

THE author of this work needs no formal introduction to the readers of this Journal. Everybody knows who Prof. James Y. Simpson is. His exalted position as an eloquent and erudite teacher, his obstetric and medical writings, his numerous contributions to the periodical press, his miscellaneous essays, and his discovery of chloroform as an anæsthetic agent, have secured for him a world-wide reputation, and made his name illustrious among men both in and out of the profession. His recent treatise on acupressure gives him fresh claims to the consideration, if not the gratitude of mankind. It affords additional proof, if any had been wanting, of the ever-active, restless, inquisitive state of a great mind, constantly in quest of new truths, and the development of new facts. It is not surprising that a man of such intellect and versatility, of such extraordinary mental gifts, as Prof. Simpson unquestionably possesses, should occasionally, like an erratic star, wander from his own more legitimate orb into the regions of another, to enrich it with new light. The case finds, singularly enough, its parallel in that of the great Paré, the illustrious inventor of the ligature, who, like the Scotch professor, was at once physician, surgeon, obstetrician, author, and reformer.

Acupressure, as a distinct hemostatic process, is a recent invention. The first account of it was communicated to the Royal Society of Edinburgh, at their meeting on the 19th of December, 1859. The paper was soon after published in the proceedings of that Society, as well as in the *Edinburgh Medical Journal*, the *London Medical Times*, and other prominent periodicals. The news of what professed to be so important an invention rapidly crossed the Atlantic, and attracted the attention of the great surgeons of this continent.

It is not our intention here to describe the operation of acupressure; to do so would evidently be out of place, as a sufficiently accurate account of it is to be found in every recent work on surgery; our chief business is with the treatise itself, and the actual progress of acupressure, as a hemostatic agent, and its probable ultimate fate.